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Decision aid on disclosure of mental health status to an employer: feasibility and outcomes of a randomised controlled trial

Claire Henderson, Elaine Brohan, Sarah Clement, Paul Williams, Francesca Lassman, Oliver Schauman, Lisa Dockery, Simone Farrelly, Joanna Murray, Caroline Murphy, Mike Slade and Graham Thornicroft

Background

Many mental health service users delay or avoid disclosing their condition to employers because of experience, or anticipation, of discrimination. However, non-disclosure precludes the ability to request 'reasonable adjustments'. There have been no intervention studies to support decision-making about disclosure to an employer.

Aims

To determine whether the decision aid has an effect that is sustained beyond its immediate impact; to determine whether a large-scale trial is feasible; and to optimise the designs of a larger trial and of the decision aid.

Method

In this exploratory randomised controlled trial (RCT) in London, participants were randomly assigned to use of a decision aid plus usual care or usual care alone. Follow-up was at 3 months. Primary outcomes were: (a) stage of decision-making; (b) decisional conflict; and

(c) employment-related outcomes (trial registration number: NCT01379014).

Results

We recruited 80 participants and interventions were completed for 36 out of 40 in the intervention group; in total 71 participants were followed up. Intention-to-treat analysis showed that reduction in decisional conflict was significantly greater in the intervention group than among controls (mean improvement -22.7 (s.d. = 15.2) v. -11.2 (s.d. = 18.1), $P=0.005$). More of the intervention group than controls were in full-time employment at follow-up ($P=0.03$).

Conclusions

The observed reduction in decisional conflict regarding disclosure has a number of potential benefits which next need to be tested in a definitive trial.

Declaration of interest

None.

Disclosure of mental illness in the employment context is a personal, complex and potentially difficult decision about which mental health professionals are frequently asked for advice. People with mental health problems frequently report or anticipate discrimination in employment.^{1–3} Anticipated discrimination by employers also forms a barrier to seeking mental healthcare; this was the most frequently reported obstacle in a recent UK study.⁴ Consequently, many people with mental health problems choose not to disclose their condition prior to being offered a job, in contrast with employers' preference.⁵ However, since the introduction of the Equality Act 2010 it is unlawful except in certain circumstances for employers in Britain to enquire about an applicant's disability or health, until that person has been offered a job or included in a pool of candidates to be offered a job when a suitable position arises.⁶ The Act does not prevent employers asking health-related questions once recruitment decisions have been taken. It is therefore the decision of the job candidate whether or not to disclose during the process of applying. There are two important legal considerations.⁶ First, a claim for direct discrimination or discrimination arising from disability can only be made where the employer knew or ought to have known that the person was disabled. Second, no duty arises to provide 'reasonable adjustments' if the employer does not know or could not reasonably be expected to know that a person has a disability. There are also non-legal considerations.⁷ Will colleagues misinterpret symptoms of mental illness, for example, as substance misuse if one does not disclose? Will someone be gossiped about, ignored or have expressions of emotion interpreted as symptoms if they disclose? How much should one say, when, and to whom?

There have been no studies of interventions to support decision-making about disclosure in the employment context. However, decision aids are commonly used to help individuals make choices between two or more medical treatment or screening options.⁸ A systematic review of 17 randomised trials found that, compared with controls, decision aids produced higher knowledge, more active participation in decision-making and lower levels of decisional conflict, which refers to uncertainty and dissatisfaction when trying to make a choice.^{9,10} Contributors to uncertainty and dissatisfaction include modifiable factors such as feeling uninformed, lack of clarity about relevant personal values, and feeling unsupported in decision-making; on the other hand, aspects of effective decision-making include satisfaction with the choice, and feeling the choice is informed, values-based and likely to be implemented. This suggests that a decision aid may be useful for people with mental health problems applying for employment; we recently developed such a decision aid called Conceal Or ReveAL (CORAL). In a previous non-randomised pilot study,¹¹ the CORAL decision aid demonstrated preliminary evidence of feasibility and reduced decisional conflict in a group of 15 service users. Ultimately we wish to determine through a randomised controlled trial (RCT) whether using CORAL leads to improvements in gaining and retaining employment. These outcomes require larger sample sizes and a longer follow-up period than are justifiable at this point.¹² We therefore conducted an exploratory RCT¹³ with the following aims: to examine several intermediate outcomes,¹⁴ to determine whether a large-scale trial is feasible; and to optimise the designs of a larger trial and of the decision aid (trial registration number: NCT01379014). We expected that, regardless of the disclosure decision, reduced

decisional conflict will allow the user of the decision aid to take more action regarding seeking employment, resulting in a greater level of empowerment and a reduction in behavioural withdrawal. Therefore our hypotheses were that, compared with the control group, people in the intervention group will show at 3 months: (a) lower decisional conflict regarding disclosure; (b) a later stage of decision-making regarding disclosure; (c) higher frequencies of employment-related actions; (d) a higher level of empowerment; and (e) lower frequencies of withdrawal in response to stigma (in any context rather than just employment).

Method

Design and setting

The trial was an individual-level single-blind RCT of the CORAL decision aid plus treatment as usual compared with a treatment as usual control. The settings were vocational services for clients with mental health problems referred from both primary and secondary care. The vocational advisors did not have direct contact with employers unless this was agreed with by a client, and the only local employers preferentially hiring people with mental health problems were some providers of mental health services; in consequence, there were no features of the setting to encourage or increase the likelihood of disclosure compared with elsewhere. The study was approved by the National Research Ethics Service Committee East of England – Essex (11/EE/0156). Progress was overseen by a trial steering committee.

Participants

Eligible service users were (a) in contact with a specialist vocational advisor working with people with mental illness; (b) referred to the advisor either from primary care or secondary mental healthcare; (c) aged 18 years or older; (d) seeking or interested in either paid or voluntary employment; (e) had a Decisional Conflict Scale¹⁵ score of 37.5 or greater (at least moderate decisional conflict) and a Stage of Decision Scale¹⁶ score of 1–5 (showing that the decision has not yet been reached); and (f) gave written, informed consent. We excluded service users who had insufficient literacy in English to use the decision aid or who lacked capacity to provide informed consent.

Recruitment, randomisation and masking

The employment advisors were asked to give study flyers to their clients and passed contact details of service users who expressed interest to the study researchers. A researcher determined eligibility using the specified inclusion and exclusion criteria, and obtained informed consent from those eligible and wishing to enter the study.

The research team was masked to the allocation sequence with randomisation performed by the independent King's College London Clinical Trials Unit. Block randomisation with randomly varying block sizes was used. Participants were stratified by: referral source i.e. primary *v.* secondary mental health services, since these groups should differ with respect to severity of illness; and length of time out of employment (less *v.* more than 12 months), as this affects the chance of regaining employment.

The research workers masked to participant status (F.L. and L.D.) were located in a separate office to those who were non-masked (E.B. and O.S.). To ensure the intervention mirrored routine practice while minimising contamination, intervention group participants were allowed to keep their decision aid and discuss it with vocational advisors but were asked not to give them or anyone else a copy. Thus, vocational advisors were not masked.

At their follow-up assessment, participants were asked not to reveal their allocation status; at the end of the interview the researcher recorded their guess about the service user's allocation status to estimate the extent of unmasking.

Intervention and control conditions

A researcher met with each participant in the intervention group, gave them the CORAL decision aid and answered any questions. The decision aid was designed for use independent from, or as an adjunct to, a clinical encounter.¹⁷ It includes six sections over 12 A4 pages: (a) the pros and cons of disclosure; (b) personal disclosure needs; (c) personal disclosure values; (d) when to tell; (e) whom to tell; and (f) making a decision. Sections b–e include service user quotes from developmental work¹¹ and the final section summarises the previous sections and asks the reader to make decisions regarding whether to disclose and if so to whom, when and what to disclose. The decision aid has a Flesch Reading Ease score of 65.6 and a revised Flesch-Kincaid Grade level of 8.4 (i.e. understandable by the average US eighth to ninth grader aged 13–15 years).¹⁰ Participants in our previous feasibility study¹⁰ took a mean of 29 min (range 11–45, s.d. = 8.8) to complete it.

We chose a care as usual control condition to answer the question of whether the use of the decision aid in addition to current standard practice is superior to standard practice alone with respect to our measures. Employment advisors often discuss disclosure needs with clients seeking competitive employment, such that usual care will often include an unstructured discussion of some of the content covered by the decision aid. Receiving help from a vocational advisor therefore provides a suitable comparator with use of the CORAL decision aid in addition to vocation advice.

Process evaluation

To assess the feasibility and acceptability of the intervention, the researcher providing the CORAL decision aid measured the time of completion and noted any content that caused confusion or dissatisfaction. To check for contamination of usual care and thus optimise future trial design, service users in the control group were asked after follow-up data collection whether they recognised the decision aid and if so who had given it to them. Qualitative interviews about the decision aid were carried out with the intervention group participants after 3-month follow-up data collection, to optimise the intervention in future; the results will be reported elsewhere.

Outcome measures

The primary outcome measure was decisional conflict, measured using the Decisional Conflict Scale (DCS).¹⁵ This measures personal perceptions of: uncertainty in choosing between options; modifiable factors contributing to uncertainty including feeling uninformed, lack of clarity about personal values and feeling unsupported in decision-making; and effective decision-making such as feeling the choice is informed, values-based, likely to be implemented and expressing satisfaction with the choice. The scale has demonstrated adequate test–retest reliability¹⁸ and has also acceptable internal consistency when used with people with a diagnosis of schizophrenia.¹⁹

The Stage of Decision Making Scale¹⁶ measures the individual's readiness to engage in decision-making. It consists of a single item with six response options from 'haven't started to think about the choices' to 'have already made a decision and am unlikely to change my mind'. Earlier stages of decision-making are associated with higher levels of decisional conflict and vice versa.¹⁶

Employment-related outcomes were assessed using a questionnaire about the occurrence and frequency over the previous 3 months of activities related to seeking, retaining and losing employment. It included disclosure to employers and requests for workplace adjustments and the outcome of such requests.

Secondary outcomes were measured using: the eight-item short version of the Work Limitations Questionnaire²⁰ for self-assessment of work performance; the self-esteem–self-efficacy and power–powerlessness subscales (17 items) of the original Boston University Empowerment Scale (BUES) used in a recent validation study;²¹ and a five-item withdrawal scale (B. Link, personal communication, 2010) based on the original nine-item subscale of the Stigma Coping Orientation scales²² to measure social withdrawal.

Sample size and power calculation

The heuristic sample size for pilot studies is 30 per group.²³ We used a larger sample to test the effect of the decision aid for a meaningful reduction in decisional conflict. The pre-post difference detected on the DCS in a preliminary study¹¹ was found to be 16.5 points (s.d. = 17.5). Estimating the effect of treatment as usual (i.e. vocational advisor support) at 4 points, this gives a standardised effect size of 0.71. Group sizes of 32 at follow-up are needed to have 80% power to detect a difference of this size or greater at the 5% significance level. Anticipating some loss to follow-up, we aimed to recruit 40 per group, i.e. a total of 80 participants.

Analysis

The intention-to-treat analysis of effectiveness compared the primary and secondary outcome measures at 3 months. Unlike the actual DCS scores, which at baseline fell above 37.5 due to the eligibility criterion, changes in DCS score were normally distributed. For change in DCS score and other normally distributed continuous outcome measures, an independent two sample *t*-test was used to compare the change in effects ($T_1 - T_0$) for the control and intervention groups, and we report the mean and standard deviation. Changes in DCS and the two empowerment subscales were then analysed using linear regression, controlling for the baseline measure of the dependent variable in each case and stratifying variables (duration of unemployment and primary v. secondary mental healthcare referral source). Those followed up and lost to follow-up were compared for employment status, DCS and Stage of Decision Making Scale scores. When continuous variables were skewed (for example number of disclosures), these were tested using a two-sample Wilcoxon rank-sum test, and we report the median and interquartile range (IQR). The differences in number of disclosures ($T_1 - T_0$) for the two groups (control and intervention) were compared. Although items such as job offers and disclosure were too infrequent to provide reliable comparisons, other items such as appointments kept with the vocational advisor and submitted applications were assessed for suitability as intermediate measures¹⁴ for future employment outcomes.

Results

Recruitment and baseline characteristics

The target sample size $n = 80$ was recruited to time. One participant subsequently withdrew consent. Two sites together provided 74% of referrals. At a third, following restructuring the team's remit moved from helping service users find employment. At the fourth, the team felt that their 'pro-disclosure' philosophy was incompatible with use of the decision aid. Baseline characteristics of the sample are shown

Table 1 Baseline demographic and clinical characteristics^a

	Total (<i>n</i> = 79)	Intervention group (<i>n</i> = 40)	Control group (<i>n</i> = 39)
Gender, <i>n</i> (%)			
Male	41 (52)	22 (55)	19 (49)
Female	38 (48)	18 (45)	20 (51)
Age, years: mean (s.d.)	36.1 (9.0)	36.6 (9.6)	35.6 (9.1)
Ethnicity (grouped), <i>n</i> (%)			
White – all	30 (38)	14 (35)	16 (41)
Black/Black British – all	37 (47)	20 (50)	17 (44)
Asian/Asian British – all	3 (4)	1 (2.5)	2 (5)
Other	9 (11)	5 (12.5)	4 (10)
Education, <i>n</i> (%)			
None	2 (2.5)	2 (5)	–
Primary school	1 (1.3)	–	1 (2.6)
Secondary school	42 (53)	22 (55)	20 (51.3)
University	28 (35.5)	12 (30)	16 (41)
Other	6 (7.6)	4 (10)	2 (5.1)
Self-report diagnosis, <i>n</i> (%)			
Schizophrenia spectrum	24 (30)	11 (27.5)	13 (33.3)
Depression	10 (12.7)	5 (12.5)	5 (13)
Bipolar disorder	13 (16.5)	7 (17.5)	6 (15.4)
Personality disorders	4 (5.1)	2 (5)	2 (5.1)
Anxiety	6 (7.6)	2 (5)	4 (10.3)
Anxiety and depression	6 (7.6)	5 (12.5)	1 (2.6)
Mixed	5 (6.3)	3 (7.5)	2 (5.1)
Don't know	11 (14)	5 (12.5)	6 (15.3)
Years in treatment, median (range)	6 (0–26)	5.5 (0–22)	6 (0–26)
Ever admitted to psychiatric ward, <i>n</i> (%)			
Yes	53 (67)	28 (70)	25 (64)
No	26 (33)	12 (30)	14 (36)
Any compulsory in-patient treatment, <i>n</i> (%)			
Yes	43 (54)	21 (52.5)	22 (56)
No	36 (46)	19 (47.5)	17 (44)
Referral source, <i>n</i> (%)			
Primary care (IAPT)	18 (23)	9 (22.5)	9 (23)
Secondary care	61 (77)	31 (77.5)	30 (77)
Length of time out of employment, <i>n</i> (%)			
Less than 12 months	22 (27.8)	11 (27.5)	11 (28.2)
12 months or more	57 (72.2)	29 (72.5)	28 (71.8)

IAPT, Improving Access to Psychological Therapies.

a. See Table 4 for employment status at baseline and follow-up.

by trial arm in Table 1. There were no substantial differences between trial arms for any variable.

Feasibility of randomisation and masking

Three control group participants reported that they had seen the decision aid before; all stated they were given it by a researcher. This was checked and found not to be the case.

Success of masking was directly estimated using the researchers' guesses as to the participants' allocation status. For the total sample, the allocation status for 46 of 71 participants was correctly guessed by the researchers (data missing for 8 participants). Assuming a null hypothesis that correct guesses as a result of chance would occur 50% of the time the observed proportion of 0.65 is significantly different ($P = 0.02$). Researchers guessed the correct allocation status in 15 of 36 intervention participants, which does not differ significantly to the proportion expected as a result of chance ($P = 0.41$). They guessed the correct

allocation status in 31 of 35 control participants, which is significantly greater than chance ($P < 0.01$).

Feasibility of the intervention and the follow-up assessments

Interventions were completed for 90% (36/40) of the intervention group. The most frequent reason for non-completion was repeated non-attendance by the participant ($n = 2$) (Fig. 1). The median time from randomisation to completion of the intervention was 12.5 days (range 2–91). The median time taken to complete the decision aid was 30 min (range 6–90). The 3-month follow-up rate was 89.9% (71/79) (Fig. 1).

Primary outcomes

No outcome measures were associated with loss to follow-up. Intention-to-treat analysis showed that decisional conflict at 3-month follow-up had fallen in both groups (Table 2), but the reduction in the intervention group was significantly greater than that in the control (mean improvement -22.7 (s.d. = 15.2) v. -11.2 (s.d. = 18.1), $P = 0.005$).

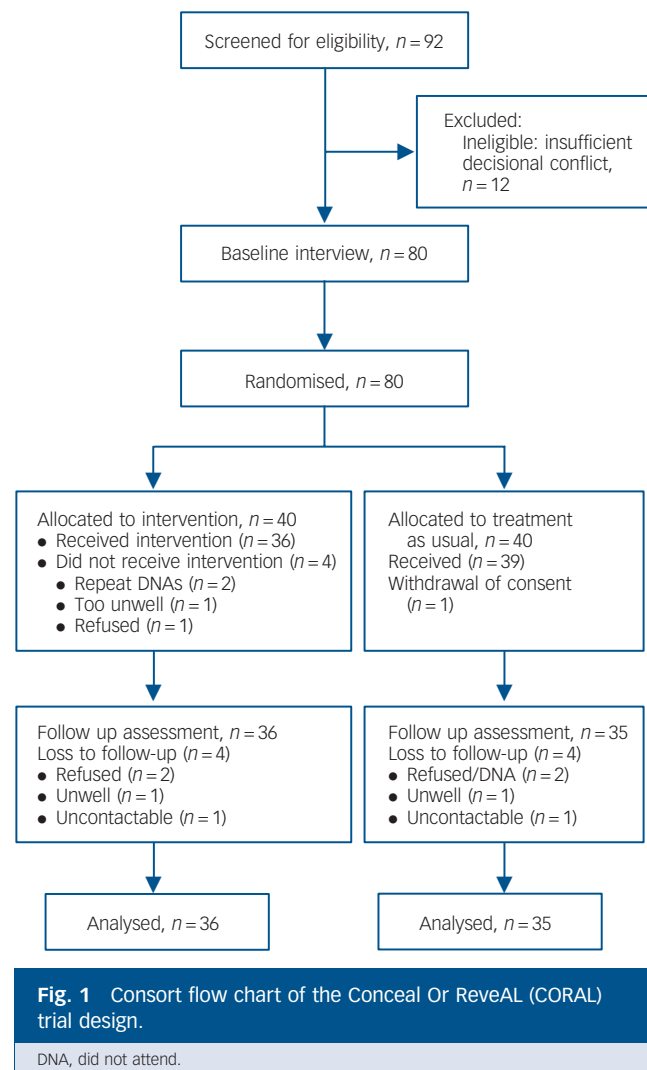
The differential improvement in DCS among the intervention group persisted after adjustment for baseline score and stratifying variables (mean improvement -11.32 , 95% CI -19.01 to -3.63 , $P = 0.005$, Table 3). Among those who provided both immediate and 3-month follow-up data ($n = 34$), there was no significant difference between mean DCS immediately after the intervention and at 3 months (32.25 (s.d. = 14.72) v. 37.04 (s.d. = 12.97) paired t -test $P = 0.06$). There was much less evidence of improvement in stage of decision-making, with no significant change between groups and no significant differences between groups at follow-up, although the changes in direction favour the intervention group (rank sum for the intervention 1348 v. expected 1296). The change between immediate and 3-month follow-up in stage of decision-making was in a positive direction (median increased from 4 (IQR 3–5) to 5 (IQR 3–6); immediate outcome data not shown in Table 2), in contrast to the negative direction of change over the follow-up period for decisional conflict; however this change was also not statistically significant (0.5, $P = 0.11$).

Secondary outcomes

Among the small numbers in employment at each time point, there were no differences between the two groups in the change in productivity costs in the past 2 weeks because of work limitations (relative to a sample with no health-related limitations), as measured by the Work Limitations Questionnaire. Unadjusted comparisons of the mean changes in the two empowerment subscales show a significant improvement on the power–powerlessness subscale (-0.20 (s.d. = 0.34) compared with -0.01 (s.d. = 0.23), $P = 0.009$), on which lower scores are more positive (Table 2). This persisted after adjustment (mean difference -0.21 , 95% CI -0.35 to -0.06 , $P = 0.006$) (Table 3). There was no significant improvement in the mean self-esteem–self-efficacy subscale score before (mean difference 0.13 (s.d. = 0.40) compared with 0.04 (s.d. = 0.28) $P = 0.282$) or after adjustment (mean difference 0.11, 95% CI -0.05 to 0.28, $P = 0.173$). There was no significant improvement in behavioural withdrawal and no significant differences between groups at follow-up.

Employment outcomes

Table 4 shows employment status of the two groups at baseline and follow-up. It should be noted that not everyone was seeing



the advisor for help finding employment in the near future; some had other short-term goals such as education and training. Comparison of the two groups at follow-up shows that significantly more of the intervention group were in full-time employment (Fisher's exact test $P = 0.03$), however this finding is based on very small numbers and should therefore be treated cautiously.

The data on employment-related actions, shown in Table 5, are skewed but appear similar in both groups at baseline and follow-up. For both groups, the proportions disclosing either verbally or in writing are relatively high, especially at follow-up and in comparison with the low proportions requesting workplace adjustments.

Discussion

This trial demonstrated that the CORAL decision aid has an impact on decisional conflict that was significant both immediately after use and at 3 months. The impact may be clinically significant because the intervention group's mean DCS at follow-up, at 37.0, was below the cut-off point for study eligibility (a score of 37.5, reflecting moderate conflict). In keeping with our hypotheses, there was also a significant improvement in the power–powerlessness subscale of BUES. The difference with respect to full-time employment in favour of the intervention

	Intervention group	Control group	Intervention group, <i>n</i>	Control group, <i>n</i>	<i>P</i>
Decisional Conflict Scale, mean (s.d.)					
Baseline	58.78 (11.72)	55.45 (12.68)	40	39	–
Follow-up	37.04 (12.97)	43.71 (15.98)	36	35	–
Difference	–22.67 (15.23)	–11.16 (18.12)	36	35	0.005
Stage of Decision Making Scale, median (IQR)					
Baseline	3 (2.5 to 3.5)	3 (3 to 3)	40	39	–
Follow-up	5 (3 to 6)	5 (3 to 5)	36	35	–
Difference	1 (0 to 3)	1 (0 to 3)	36	35	0.543
Work Limitations Questionnaire, median (IQR)					
Baseline	0.06 (0.03 to 0.13)	0.07 (0.05 to 0.11)	10	8	–
Follow-up	0.09 (0.00 to 0.13)	0.06 (0.01 to 0.08)	14	9	–
Difference	–0.001 (–0.03 to 0.01)	–0.008 (–0.04 to 0.04)	8	6	0.542
Empowerment: self-esteem–self-efficacy subscale, mean (s.d.)					
Baseline	2.74 (0.43)	2.68 (0.39)	40	38	–
Follow-up	2.87 (0.46)	2.71 (0.46)	36	34	–
Difference	0.13 (0.40)	0.04 (0.28)	36	33	0.282
Empowerment: power–powerlessness subscale, mean (s.d.)					
Baseline	2.48 (0.32)	2.49 (0.30)	40	39	–
Follow-up	2.28 (0.33)	2.49 (0.32)	36	34	–
Difference	–0.20 (0.34)	–0.01 (0.23)	36	34	0.009
Social withdrawal, median (IQR)					
Baseline	1.4 (0.8 to 2.2)	1.6 (0.6 to 2.6)	40	39	–
Follow-up	1.3 (0.7 to 1.8)	1.2 (0.8 to 2.4)	36	35	–
Difference	0 (–0.8 to 0.4)	–0.2 (–0.8 to 0.3)	36	35	0.890
IQR, interquartile range. Results in bold are statistically significant.					

group is in-keeping with our conceptual model of the intervention, however, this may be a chance finding because of the small numbers. The other changes we hypothesised would accompany reduced decisional conflict were not found. In the case of employment-related actions, their skewed distribution and small numbers made comparisons difficult. We demonstrated that a definitive trial is feasible with respect to recruitment and acceptability of the intervention to service users and vocational advisors, and collected data to improve the design of a definitive trial and of the intervention.

Decisional conflict also fell in the control group. This may be as a result of regression to the mean, particularly as a score of 37.5 or over was a study eligibility criterion. This change may also reflect the result of working with a vocational advisor providing usual care. However, the improvement in the power–powerlessness subscale of the BUES was seen only in the intervention group. There was little evidence for change in stage of decision-making among either group. This is consistent with the only difficulty expressed by intervention group participants, which was in recording a decision in the last section of the CORAL decision aid.

	Mean difference (95% CI)	<i>P</i>
Decisional Conflict Scale	–11.32 (–19.01 to –3.63)	0.005
Empowerment: self-esteem–self-efficacy subscale	0.11 (–0.05 to 0.28)	0.173
Empowerment: power–powerlessness subscale	–0.21 (–0.35 to –0.06)	0.006
Results in bold are statistically significant.		

	<i>n</i> (%)	
	Intervention group	Control group
Full time ^b		
Baseline	2 (5.0)	0 (0.0)
Follow-up	7 (17.5)	1 (2.6)
Part time		
Baseline	3 (7.5)	2 (5.1)
Follow-up	4 (10.0)	4 (10.3)
Volunteer (unpaid)		
Baseline	9 (22.5)	5 (12.8)
Follow-up	7 (17.5)	4 (10.3)
Looking for a job		
Baseline	13 (32.5)	16 (41.0)
Follow-up	10 (25.0)	16 (41.0)
Would like to work but afraid of loss of benefits		
Baseline	4 (10.0)	2 (5.1)
Follow-up	2 (5.0)	2 (5.1)
Not able to work (disabled)		
Baseline	5 (12.5)	8 (20.5)
Follow-up	3 (7.5)	5 (12.8)
I choose not to work		
Baseline	1 (2.5)	1 (2.6)
Follow-up	1 (2.5)	1 (2.6)
Student		
Baseline	3 (7.5)	4 (10.3)
Follow-up	3 (7.5)	3 (7.7)
Missing		
Baseline	0 (0.0)	1 (2.6)
Follow-up	3 (7.5)	3 (7.7)
^a Including one full-time worker and one student in the intervention group and one full-time worker in the control group who were not able to attend the follow-up interview for employment or study-related reasons.		
^b Fisher's exact test <i>P</i> =0.03.		

Table 5 Employment-related outcomes at 3 months

	Intervention group	Control group	Intervention group, <i>n</i>	Control group, <i>n</i>
Appointments arranged with vocational advisor, median (IQR) (range)				
Baseline	2 (1–6) (0–12)	3 (2–6) (1–16)	39	39
Follow-up	1 (1–3) (0–12)	2 (1–4) (0–12)	36	35
Number of arranged appointments attended, median (IQR) (range)				
Baseline	1 (1–1) (0–1)	1 (1–1) (0.5–1)	35	39
Follow-up	1 (1–1) (0–1)	1 (1–1) (0.5–1)	25	27
Number of job applications (both paid and voluntary) made, median (IQR) (range)				
Baseline	1 (0–10) (0–120)	2 (0–5) (0–1500)	39	38
Follow-up	1 (0–3) (0–50)	2 (0–6) (0–1000)	36	35
Number of job interviews, median (IQR) (range)				
Baseline	0 (0–1) (0–4)	0 (0–1) (0–15)	40	39
Follow-up	0 (0–1) (0–2)	0 (0–0) (0–4)	36	35
Number of jobs offered, median (IQR) (range)				
Baseline	0 (0–0.5) (0–3)	0 (0–0) (0–3)	40	39
Follow-up	0 (0–1) (0–2)	0 (0–1) (0–2)	36	35
Number of jobs accepted, median (IQR) (range)				
Baseline	0 (0–0) (0–3)	0 (0–0) (0–2)	35	32
Follow-up	0 (0–1) (0–2)	0 (0–1) (0–1)	27	23
Verbal disclosure during the application/interview/job offer process, <i>n</i> with a yes response (%)				
Baseline	5 (18)	3 (11)	28	28
Follow-up	6 (24)	9 (39)	25	23
Written disclosure during the application/interview/ job offer process, <i>n</i> with a yes response (%)				
Baseline	7 (24)	3 (10)	29	29
Follow-up	8 (32)	5 (22)	25	23
Requests for workplace adjustments, <i>n</i> with a yes response (%)				
Baseline	1 (4)	0 (0)	27	26
Follow-up	0 (0)	1 (5)	25	19
Job loss, <i>n</i> with a yes response (%)				
Baseline	2 (6)	3 (8)	33	36
Follow-up	2 (6)	0 (0)	31	30
Work-related training, <i>n</i> with a yes response (%)				
Baseline	14 (36)	11 (28)	39	39
Follow-up	14 (39)	12 (34)	36	35

Strengths and limitations

In addition to collecting data to optimise the design of both a definitive trial and the CORAL decision aid, this trial tested several intermediate outcomes¹⁴ hypothesised to improve as a result of the intervention. The selection of a sample in which decisional conflict was at least moderate allowed us to assess the effectiveness of the decision aid with respect to this outcome in a suitable group. The chief limitation is the small numbers and skewed distributions of employment-related activity among the sample. These make it difficult to estimate effect sizes for employment-related outcomes and thus to select an appropriate primary outcome measure for use in a definitive trial. A sample with a higher proportion actively seeking competitive employment would therefore facilitate testing of the decision aid with respect to this outcome. It would also be informative in a definitive trial with such a sample to ascertain the type of occupation participants are seeking and obtain during the follow-up period, and what their disclosure experiences are at all stages, from during the application process to having become unwell in employment. Given that follow-up of clients once in employment is an important part of fidelity to the supported employment model, recruitment from high-fidelity services would allow the impact of the decision aid to be assessed among those already working who have not yet decided whether to disclose.

Compared with many mental health services study samples, the sample was relatively young and well educated. We suspect

that this reflects referral patterns to the vocational services concerned and in a future trial would aim to collect data to show the extent to which the sample is demographically representative of the services' clientele.

We did not assess the cost-effectiveness of the decision aid for two reasons. First, the methods for data collection for service and societal costs we would use in a definitive trial have been well tested. Second, since the current trial could only test intermediate outcomes, we were also unable to determine cost-effectiveness with respect to an employment-related primary outcome measure. We did, however, collect data to allow us to identify any extra resources for example time required to implement the intervention, to which costs can be attached for example for implementation by employment advisors. Developmental costs of the intervention could have been included, but apportioned over a large number of people these would be minimal.

Implications for research and practice

We identified a number of potential improvements to optimise the intervention and the design of a future trial. Besides a larger sized sample, the design of a future definitive trial would need to consider recruitment of a sample with a higher proportion actively looking for employment, in addition to a longer follow-up period for employment outcomes to accrue. Second, cluster randomisation at the level of the vocational advisor would allow

the study of the administration of the decision aid by vocational advisors, and thus determination of the effectiveness of the intervention in routine care, while also avoiding contamination among controls. Manualisation will be needed for delivery by vocational advisors, in keeping with the delivery of other complex interventions in general and specifically with other interventions to promote greater involvement in care such as self-management²⁴ and advance statements.^{25–29} Third, an active control should be considered as researchers were able to guess control allocation status in most cases. This appeared to be due to the intervention group participants' greater level of familiarity with the outcome measures and with the instructions that they must not disclose their status, as they had had an additional meeting for intervention delivery and immediate outcome data collection. A suitable active control could be an information leaflet about the rights of people with disabilities under the UK Equality Act 2010, again administered by a vocational advisor. A future trial should seek to identify variables mediating any relationships between reduced decisional conflict and employment outcomes, for example well-being, empowerment and intended and/or actual disclosure, and requests for and implementation of reasonable adjustments. Finally, future economic evaluation would be affected by the economic and labour market conditions present,³⁰ which therefore require description.

Vocational advisors and health professionals that asked for advice about disclosure may find the CORAL decision aid helpful, as it is the only structured tool based on both theory and evidence to address this difficult decision. Use of the tool is predicated on the service user's right to choose whether to disclose, which should not be eroded through implementation of employment programmes that mandate disclosure or pressure service users to disclose. Rather, to be maximally effective, programmes must acknowledge the difficulty of this decision⁷ and support service users in making it. Currently we have no evidence to support its use in the absence of seeing a vocational advisor. However, given that vocational advisors are scarce in many countries, the effectiveness of the CORAL decision aid as a standalone tool is a worthwhile subject for future research.

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poems
by
doctors

This Child

Miriam Brown

Look at this child;
see the hand that strikes her,
watch her shoulders wilt,
glimpse her diffident plea.

Taste for this child;
tire of her insipid fear,
sip tentatively others' asperity,
become jaded by nausea.

Feel for this child;
reach for her in her distance,
bounce off her armour,
be cut by her latent scars.

Listen to this child;
hear her laughter echo and fade,
catch the catch in her throat,
heed her cry.

For this child
become astute.
Discover her secret
and acknowledge for her
the chafe of misplaced guilt.